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XV. *Experiments made with the View of decomposing Fixed Air, or Carbonic Acid.* By George Pearson, M. D. F. R. S.

Read May 24, 1792.

FROM a Paper read to the Philosophical Society of Edinburgh, in 1755, published in the second volume of the *Physical and Literary Essays*, Doctor BLACK appears to have discovered the affinities between an æriform substance, which he called *fixed air*, and alkalies, quick-lime, and magnesia. His experiments also shewed, that many properties of these bodies depended upon the union and separation of this air. The discovery of these facts established this elastic fluid to be a peculiar species of substance.

Mr. CAVENDISH, Dr. BROWNRIGG, Dr. PRIESTLEY, Sir TORBERN BERGMAN, Mr. BEWLEY, Mr. KIRWAN, and other chemists, afterwards extended, very considerably, the history of fixed air. The question, whether it was a simple or compound body, was discussed; and by many persons it was believed to have been proved, that fixed air was composed of phlogiston and respirable air. But some of the principal facts, upon which this belief was founded, being afterwards demonstrated to be erroneous; and the production of fixed air being, to the apprehension of many chemists, more satisfactorily accounted for by the new principles of chemistry, this doctrine of its composition became no longer tenable. As the science of chemistry ad-

vanced, many acids were demonstrably proved to consist of a peculiar basis, and respirable air ; and on the ground of analogy it was concluded, that all other acids were composed in a similar manner. Fixed air having been shewn, by Mr. BEWLEY, and by BERGMAN, to be an acid, of course its composition was considered, in the new system of chemistry, to be similar to that of all other acids. On examining facts already well ascertained, and by various experiments discovering others, no clear instance could be perceived of the *formation* of fixed air, but in those cases where charcoal was applied red hot to respirable air. Mr. LAVOISIER at last established this interesting fact, by a conclusive experiment, published in a volume of the *Memoirs of the Academy of Sciences* in 1781, and in his *Traité Élémentaire* in 1789, by which he demonstrated that charcoal of wood, except a minute portion of residue, as might reasonably be expected, combined with respirable air, and composed fixed air only. This is the date, therefore, of the discovery by synthesis, of the composition of fixed air ; or, as I would rather call it, with Mr. LAVOISIER, carbonic acid. The proof by analysis, however, was required, to render the demonstration of the composition of this elastic fluid complete. The honour of the first analytical experiments on carbonic acid is due to Mr. TENNANT, F. R. S. who, in a paper read to this Society, in March, 1791, and contained in volume LXXXI. of the *Philosophical Transactions*, asserted, that charcoal and phosphoric acid were produced by applying phosphorus to red hot marble ; from which he inferred, that the carbonic acid of the marble was decomposed. This decomposition, the ingenious author conceives to be effected by the united powers of affinity between phosphorus and the respirable air of the carbonic acid

in the calcareous earth, and between the phosphoric acid, thus composed, and the quick-lime of the calcareous earth. That the black matter produced is really charcoal, the author has proved by adequate experiments. The inference, however, does not appear to me to be just, that the charcoal and phosphoric acid are the necessary result of the agency of the affinities, as stated by Mr. TENNANT. For the well known fact, that phosphorus cannot be produced from bone-ashes by the application of charcoal and heat, I think, only proves that the powers of affinity between respirable air and phosphorus, together with the affinity between the compound formed by their union (namely, phosphoric acid,) and quick-lime, are *not inferior* to the joint affinities between the respirable air, in the phosphoric acid, and charcoal, and between the compound of respirable air and charcoal (namely, carbonic acid) and quick-lime. Hence, from the principle referred to, it could not be concluded, that carbonic acid, combined with quick-lime, would be decomposed by phosphorus attracting its respirable air, and the phosphoric acid, thus formed, attracting the quick-lime. Experience only could determine the result of these affinities, but no proof has been given, from the examination of the mixture, after applying phosphorus to red hot marble; such as finding that carbonic acid was really decomposed, because there was a deficiency of this elastic fluid, and that the charcoal and phosphoric acid corresponded to this deficiency. Accordingly some chemists have conjectured that the small quantity of charcoal afforded in this experiment pre-existed in the phosphorus, which, it is well known, is distilled from charcoal; and others have suspected that it might have arisen from accidental impurities.

As experience, also, has taught us that phosphorated mineral

alkali will not yield phosphorus by exposure to charcoal and heat, unless *plumbum corneum* be added, we cannot infer that the carbonic acid in mild mineral alkali will be decomposed by phosphorus ; because, as in the case of bone-ashes and phosphorus, the joint affinities between respirable air and phosphorus, and between phosphoric acid and mineral alkali, are, by this fact, shown to be *not inferior* to the conjoined affinities between charcoal and respirable air, and between carbonic acid and that alkali. No other conclusion can be drawn with respect to the affinities exerted when charcoal is applied to phosphorated vegetable alkali ; because the affinity is stronger between the phosphoric acid and vegetable alkali, than between the same acid and mineral alkali. As the attractive forces between phosphoric acid and barytes, and between that acid and magnesia are, very probably, at least equal to those between phosphoric acid and fixed alkalies, the question, whether carbonic acid united to these earths can be decomposed by phosphorus, remains to be determined by experiments. But with respect to the volatile alkali, we know, by the experience of making phosphorus from urine, that the united affinities between respirable air and phosphorus, and between phosphoric acid and volatile alkali, are *inferior* to the joint affinities between charcoal and respirable air, and carbonic acid and volatile alkali ; hence, in a due degree of heat, phosphorus and mild volatile alkali are formed from phosphorated volatile alkali and charcoal, consequently carbonic acid combined with volatile alkali, cannot be decomposed by phosphorus and heat, even if the volatility of that alkali did not, apparently, render it impossible to apply the requisite degree of heat. We know so little of the degree of chemical attraction betwixt clay

and phosphoric acid, that the question, whether carbonic acid united to clay will be decomposed by phosphorus ; can only be answered by future experiments.

As I presume that I have made experiments which enable us to draw conclusions concerning the above cases of compound attraction, and which also shew, in several instances, that carbonic acid is decomposed, and affords respirable air, and charcoal ; I think it my duty, on a subject so very interesting in the present state of chemistry, to submit them to the consideration of this Society.

Experiments with Phosphorus, applied to mild fossil Alkali.

I began with attempting to decompose carbonic acid in union with fossil alkali, in preference to the same substance combined with quick-lime, because the proportion of this elastic fluid is much greater in mild fossil alkali than in calcareous earth, because the affinity is not so strong between carbonic acid and fixed alkalies, as between carbonic acid and quick-lime, and because the mechanical separation of charcoal from alkalies, and phosphorated alkalies, may be more easily made than of charcoal from calcareous earth and phosphoric selenite. The purest fossil alkali I could procure was employed, from which I had expelled $\frac{67}{100}$ of its weight of water, but none of its carbonic acid.

Into a thick white glass tube, almost one inch wide, three feet and a half in length, coated within nine or ten inches of the open end, were introduced two hundred grains of transparent phosphorus, and eight hundred grains of the above de-aquated alkali were pressed down upon them. The tube, thus

charged, was then bent so that the open end might be kept conveniently plunged in quicksilver during the experiment. The coated part of the tube, containing the alkali, excepting two or three inches next to the phosphorus, was gradually heated over a portable furnace till it was red hot, and rather flexible, in which state the part containing the phosphorus was gradually drawn over the fire, and kept red hot twenty minutes. At the beginning of the experiment, quicksilver rose several inches within the tube, and when the coated part grew hot, phosphorus was sublimed into the upper and cool part of it: about twenty drops of water were condensed over the quicksilver; and two ounce measures of phlogisticated air, with a little respirable air, which had the smell of phosphorus, came over. The tube, when cold, being broken, the lower part was found to contain a loosely-cohering solid, as black as charcoal, which weighed 428 grains, and above this, a grey and white substance, partly fused, and partly in a powdery form, which, with adhering glass, weighed 358 grains. Neither in this, nor in other similar experiments, was I able to collect the whole contents of the tube, without glass which had been melted, that adhered to the alkali, on which account I could not determine accurately the total weight, independently of glass; but I was sure, from a number of trials, that it was a little less than the original weight of the alkali. The phosphorus, sublimed into the upper part of the tube, was moist from the adhering phosphoric acid: it was inflamed by slight friction, *viz.* merely on breaking the tube.

The 428 grains of black alkaline matter thus obtained, afforded, by solution in boiling hot concentrated acetous acid, a little more than 25 ounce measures of carbonic acid, under the mean pressure of the atmosphere, and of the temperature of

45°; that is, 100 grains of the black matter yielded about six ounce measures of this elastic fluid. In other similar experiments, the quantity of carbonic acid varied from four to seven ounce measures in 100 grains of this blackened alkali; except in one experiment, which afforded only three ounce measures of the acid, but the largest proportion of charcoal I ever made, namely, 12 grains.

The solution of the above 428 grains was filtered, and the residue, which was black, was lixiviated with boiling distilled water. This residue, when dried, weighed 32,4 grains; it had no taste or smell, but was an impalpably fine, intensely black, and very light powder; for it occupied a one ounce-and-a-half measure, therefore, may be estimated to have been about 22 times lighter than water. A little of this black powder, being thrown upon a red hot iron plate, ignited readily, but left a residue, which was one-fourth of its weight: this being again thrown upon the red hot iron plate, it ignited, and there remained, on cooling, a very small portion of brownish powder, which diminished to almost nothing, by being applied twice more to the iron kept red hot for several minutes. On sprinkling this black powder upon boiling nitre, it sparkled most brilliantly and detonated, leaving a colourless mass entirely soluble in water. This black powder, mixed with powdered nitre, deflagrated on exposure to heat, in a retort with the air-apparatus affixed to it, affording, over quicksilver, carbonic acid. This black matter, also, reduced the calx of lead; being mixed with tartar of vitriol, and heat being applied, *bepar sulphuris* was produced; and with phosphoric acid, phosphorus was obtained. That, therefore, these 32,4 grains were charcoal, cannot, I think, be doubted. I might

add, that accidentally I found this powder, red hot, decomposed water as charcoal does.

The above filtered liquid was evaporated to one pint, and shewed signs of acidity: to it was added muriated lime till it produced no further precipitation. The dried precipitate weighed 130 grains, and was found to be phosphoric acid combined with lime; and the liquor, in which this precipitation took place, was ascertained to be muriated and acetated fossil alkali, with a little redundant acetous acid, and a small portion of phosphoric selenite.

The grey and white alkaline matter, with bits of melted glass, weighing 358 grains, as abovementioned, by solution in concentrated acetous acid, afforded 41 ounce measures of carbonic acid, and a residue upon the filter, which, when well dried, weighed 44 grains. This residuum consisted of rough, sharp-pointed, black and white particles; it was much specifically heavier than the residue of the other part of the alkaline matter abovementioned to have been examined; it deflagrated a little on being thrown upon boiling nitre, but left above $\frac{4}{5}$ of its weight of matter insoluble in water, and which I supposed was vitrified. The filtered liquor from these 358 grains of alkaline substance yielded to the precipitant muriated lime 21 grains of phosphoric selenite.

To satisfy myself still further, that carbonic acid had been destroyed in this experiment, and to form some estimate of the quantity which had disappeared, I separated it, by concentrated acetous acid, from 400 grains of mild alkali, taken from the same parcel as that was which afforded charcoal, and I found the quantity to be 104 ounce measures, or 26 ounce measures in each 100 grains of mild alkali.

To afford a still more decisive proof that carbonic acid had not been combined, or escaped, in this experiment, but had been destroyed, I exposed some of the same parcel of alkali which had afforded charcoal to the same degree of heat, in tubes, under similar circumstances to those in the above experiment ; and I found that no carbonic acid, but a little water, came over into the air-apparatus ; that the total weight of the alkali was diminished, but that a given weight of it, after the experiment, afforded rather more carbonic acid, by solution in acetous acid, than an equal weight of the same parcel of alkali not thus subjected to heat. This diminution of weight of alkali, and greater proportion of carbonic acid, I impute to the water visibly separated in the glass tubes, and, perhaps, also absorbed in the earthen ones. Accident afforded a still more decisive proof of the decomposition of carbonic acid. In the beginning of the experiment, the tubes sometimes cracked about four or five inches from the part containing the phosphorus: on cooling, I found, in the part below the crack, black alkaline matter, which yielded much less carbonic acid than the same weight of alkali before the experiment ; whereas the alkali above the crack was white, and contained the same quantity of this elastic fluid that it did before it was exposed to heat.

In the experiment above particularly described, it appears that in one part of the alkali there was a deficiency of twenty ounce measures of carbonic acid *per cent.* of alkali ; but a production of rather more than eight grains of charcoal, and of as much phosphoric acid as formed about thirty grains of phosphoric selenite ; the composition of which may be estimated to be, of phosphorus, five grains ; respirable air, ten grains ; and quick-lime, fifteen grains. Now, as it has been

demonstrated by M. LAVOISIER, that charcoal, either totally, or a minute proportion excepted, combines with respirable air, and forms carbonic acid; and other familiarly known, though less accurate, experiments, show that carbonic acid is formed whenever charcoal and respirable air are applied to each other in a due degree of heat; and as there are no other sources perceivable of respirable air and charcoal in this experiment, it seems to prove decisively that they are derived from the carbonic acid, which is decomposed by the superior affinities between phosphorus and respirable air, and phosphoric acid and alkali, to those between respirable air and charcoal, and carbonic acid and alkali. An additional proof of the reality of this decomposition is afforded by the examination of the 358 grains of white and grey alkaline matter, of the same experiment, which afforded much more carbonic acid, and much less charcoal and phosphoric acid. I am very fully aware that the proportions of respirable air and charcoal, produced in this experiment, do not correspond to the proportions of them we should have expected, consistently with the synthetical experiments concerning carbonic acid. The variation is especially great with respect to respirable air, of which there should have been eighteen grains instead of five, to combine with the whole of the charcoal, but, from the nature of the experiment, we cannot even approximate to the truth with respect to the real quantity of respirable air produced; for the phosphorus which sublimed probably carried off a little of this air, some of the phosphoric acid thus formed fused along with alkali and glass, and some phosphoric selenite remained dissolved in the liquid. Supposing the whole of the charcoal formed in this experiment to be united to respirable air, the quantity of carbonic acid composed may be calculated to be 104,

grains ; for 32 grains of charcoal combined with 72 of respirable air compose 104 grains of carbonic acid, or 70 ounce measures ; to which must be added the 25 ounce measures of undecomposed carbonic acid separated. Then the quantity of this elastic fluid, calculated to be decomposed, and remaining united in about 400 grains of mild fossil alkali, is 95 ounce measures, and the quantity of it actually found to exist in an equal weight of alkali, was about 112 ounce measures : therefore the quantity of charcoal produced does not differ very considerably from that calculated to be contained in the carbonic acid decomposed. But future experiments must determine whether there is a like coincidence with respect to the other supposed constituent of carbonic acid, namely, respirable air.

I deem it unnecessary to relate a number of experiments which I have made, the result of which was similar to the preceding one ; but it may be proper to mention, that in every instance, the proportions of phosphoric acid and charcoal were inversely as the quantity of carbonic acid remaining in the alkali ; and that the quantities of these two products diminished as the quantity abovementioned of phosphorus was diminished ; accordingly, the alkali most exposed to the phosphorus contained the greatest proportion of charcoal.

I made this experiment several times with alkali, which contained a good deal of water, and then I obtained a large quantity of air, which smelt of phosphorus, but did not explode on contact with atmospheric air ; it contained no carbonic acid, nor phlogisticated air, excepting a few ounce measures in the first jar that came over, but it exploded loudly when mixed with an equal bulk of dephlogisticated air, on applying a

lighted wax taper. A charge of 95 grains of phosphorus, and 540 grains of the above alkali, afforded 206 ounce measures of this inflammable air, which was of the same quality whether it was received over water or quicksilver. This air, I apprehend, was produced by the decomposition of the water in the alkali, in consequence of the superior affinity between phosphorus and respirable air, to the affinity betwixt respirable and inflammable air. Therefore when moist alkali is used, *cæt. par.* more phosphoric acid will be formed than when dry alkali is employed; and in calculating the quantity of respirable air formed, regard must be paid to the decomposition of water. It appears also, that it requires less heat to decompose water by phosphorus, than to disunite carbonic acid from fixed alkali.

In these experiments I frequently used thick white glass tubes, and applied heat for a long time, to the degree of rendering them flexible: when cold, I found the internal surface in contact with the black alkaline matter full of cells, or small cavities, and rough, to which small grains of lead adhered, consequently the respirable air of the calx of this metal in the glass had been attracted, and reduction effected. This reduction might be produced by three substances here present, namely, phosphorus, inflammable air, and charcoal; but I impute it to the charcoal; 1st. because I obtained no particles of lead by passing phosphorus through a tube filled with powdered white glass, heated to the degree of rendering the tube soft, although on cooling, I found the internal surface of the tube was turned black, which colour could not be removed by rubbing, or by acids. This appearance I cannot explain. 2dly. This reduction is effected when there is no

water present, at least when no inflammable air is extricated. gdy. The greatest quantity of regulus of lead was obtained in those parts of the alkaline matter which contained the smallest quantity of charcoal, and therefore, I conceive, the charcoal, formed in those parts, had united to the air of the calx after the phosphorus had been driven through the alkali, so that the carbonic acid thus composed could not be decomposed, but was combined with the alkali, which was always redundant. In calculating the proportion of carbonic acid decomposed, it will be necessary to consider the reduction which here takes place.

If the air-apparatus be not affixed to the tube, containing a charge of phosphorus and alkali, charcoal and respirable air will be formed ; but the phosphorus will take fire at the open end of the tube, and burn with splendour, as in dephlogisticated air.

Porcelain, or well glazed WEDGWOOD tubes, answer in these experiments better than glass ones, the insides of which are apt to melt ; but unglazed vessels allow the phosphorus to pass through their pores, though part of the carbonic acid may be decomposed.

The heat applied must be greater than the white glass now made can endure without melting ; for, I passed phosphorus through a tube containing mild fossil alkali, heated so that it appeared red hot in the dark, and no charcoal was formed, though the inside of the tube was blackened.

Experiments with Phosphorus applied to mild vegetable Alkali, calcareous Earth, Barytes, Magnesia Alba, and Clay.

Similar experiments to the preceding, made with mild alkali of tartar, in place of fossil alkali, afforded, apparently, as much charcoal, and which was easily obtained, but as the phenomena were similar, and as I have not ascertained with any tolerable precision the proportion of the carbonic acid decomposed, and of the products, it is unnecessary to give any further account of them.

By the like experiments, I endeavoured to decompose the carbonic acid in calcareous, barytic, magnesian, and argillaceous earths. The matter remaining in the tubes, after exposure to heat, was blackish, and grey, seemingly from charcoal being formed, though in much smaller quantity than in the preceding experiments with fixed alkalies. For the reasons above given, I omit the particulars of these experiments on earths.

It appears to me, that the above experiments justify the inference that the joint affinities between respirable air and phosphorus, and between phosphoric acid and mineral alkali, are superior to the affinity between the whole, or at least part, of the respirable air of carbonic acid and charcoal, co-operating with the affinity between that acid and the same alkali. And, although I have not ascertained the facts with equal satisfaction, the experiments already made seem to warrant the conclusion, that the order of the affinities is such, that carbonic acid united to vegetable alkali, lime, barytes, magnesia, and clay, will be decomposed by phosphorus in a due degree of heat. With respect to carbonic acid combined with

volatile alkali, as might be expected, I could not decompose it, although I transmitted boiling hot phosphorus through a very long tube, containing mild volatile alkali.

Experiments with Phosphorus applied to Quick-lime, and caustic fixed Alkalies.

I need not explain that these experiments must confirm or invalidate the conclusion above drawn, that carbonic acid was decomposed by phosphorus applied to mild alkalies, and earths which contain this elastic fluid.

As the quick-lime which can be procured in London must contain both water and carbonic acid, I exposed a quantity of this earth 48 hours to the fire of a reverberatory furnace, by which it was contracted to half its former bulk, and was diminished in its weight; it was, however, still soluble in acids, and afforded no carbonic acid. In the manner above described, I exposed 240 grains of it, with 60 grains of phosphorus, to heat in a coated glass tube. On breaking the tube, when cold, I found at the bottom about 30 grains of blackish and white powder; and above that, to the extent of four or five inches, was a rose-coloured powder, which by its contact with air soon became of a reddish brown colour; above this was the quick-lime, scarcely altered in its colour, but it had, like the rest of the powder in the tube, an alliaceous smell. On tasting a little of this reddish powder, I was surprised by its exploding upon my tongue. I threw a few grains of it into several ounces of cold water, it did not seemingly dissolve, or turn black, but in a few minutes emitted air-bubbles, which rose to the surface of the water, and then burst and exploded,

producing a white circular cloud, which in ascending expanded gradually, till it burst in the air. It continued to emit these bubbles from time to time, during an hour, and then left a grey sediment, which was phosphoric selenite and lime, and the water tasted strongly of lime. The same powder, in hot water, exploded more rapidly and loudly than in cold, but not so violently as the phosphoric air obtained by boiling phosphorus in a lixivium of caustic fixed alkali. By putting this powder into an inverted jar of water, I collected a quantity of the air which it produced; it had the properties of the phosphoric air already mentioned, and, amongst others, by standing over water a day or two, it became no longer spontaneously inflammable, but appeared to have deposited phosphorus upon the water and sides of the vessel, and exploded upon applying to it a lighted wax taper. This powder, therefore, I apprehend, consists of phosphorus and lime united by heat; it readily decomposes cold water, and then the inflammable air disengaged unites with, or rather suspends, a portion of phosphorus, and forms phosphoric air. The phosphorus thus suspended by standing is deposited, and inflammable air alone remains; the other constituent of water, respirable air, unites to another portion of phosphorus, and composes phosphoric acid, which combines with lime, and forms phosphoric selenite. This compound of lime and phosphorus, which some of my chemical friends have called *fulminating bepar of phosphorus*,* may be used to obtain phosphoric air with much more ease than by the usual method. †

* In the new system of chemistry it will be called *phosphur of lime*.

† Dr. INGENHOUSZ has devised some surprising and beautiful experiments with this substance.

This experiment seems decisive, that the charcoal afforded in the former ones was derived from carbonic acid.

My next experiment was with caustic alkali and phosphorus. The caustic vegetable alkali I employed was blackish, partly from a very small quantity of calx of iron, and partly, I think, from other causes which I do not understand; and I was not able to prepare myself, or obtain from others, fixed caustic alkali in a solid form which was colourless. It also always contained a small quantity of carbonic acid. I introduced into a glass tube 50 grains of phosphorus, and 150 of pulverized caustic vegetable alkali, previously found to contain three ounce measures of carbonic acid in each 100 grains. This charge was exposed to heat, as in the former experiments. On breaking the tube, when cold, the alkaline matter was blacker than before: a little of it thrown into hot water emitted bubbles of phosphoric air, but not in cold water: in rubbing off this alkali from the sides of the tube some pieces of it took fire. I dissolved as much as I could of this black alkaline matter, by pouring boiling water upon it on a filter: a greenish lixivium passed through first, then a less coloured alkaline liquor; and last of all, limpid water. A residue left upon the filter being dried, weighed ten grains; it was a blackish brown, impalpable powder, at least five times specifically heavier than the charcoal obtained in the preceding experiments.

(a) Six grains of this residue upon a thin plate of iron, heated over a candle, burnt with a green and blue flame, emitting a somewhat arsenical odour, and it did not remain ignited after the flame ceased. A coal-like matter was left, which weighed three grains.

(b) These three grains (a) being placed upon an iron plate, red hot, again emitted a little green and blue flame, with the like, but a weaker smell than before; the substance remaining continued ignited but a few seconds of time, though the iron was red hot much longer. The residuum, which was black, weighed two grains and a half.

(c) The residuum (b) was exposed in a silver spoon red hot; it soon ignited, and sparkled; but though this heat was applied six minutes, on cooling, a blackish matter remained, which weighed 1,3 grains.

(d) The 1,3 grains of residue (c), under the flame applied with the blow-pipe, gave some indications of fusion, yet it remained black: but

(e) Being thrown into boiling nitre, a slight detonation ensued; this salt was not coloured by it, and it was dissolved in water, leaving scarcely a visible quantity of matter upon the filter.

I think I may safely conclude that but a small part of these ten grains of residue was charcoal: and as the proportion is so much smaller than has been shown to be afforded by an equal weight of alkali saturated with carbonic acid, this experiment confirms the conclusion, that the charcoal produced in the preceding experiments is from the decomposition of that elastic fluid. The small quantity of charcoal in the above ten grains of residuum was, perhaps, intimately mixed with phosphorus and alkali; but more experiments are required to determine satisfactorily the nature of this compound.

To corroborate the inference concerning the source of the charcoal above described, I add, that not a grain of it was produced by applying phosphorus to vegetable alkali and fossil alkali saturated with vitriolic and marine acid.

The resemblance betwixt phosphorus and sulphur, induced me to consider whether carbonic acid, combined with alkalies and earths, might not be decomposed by sulphur. Experience, however, did not favour the supposition of a decomposition in these instances ; for it is well known that hepar may be formed by applying charcoal to tartar of vitriol, GLAUBER'S salt, vitriolic selenite, and ponderous spar: and therefore that the affinity between charcoal and respirable air is superior to the joint affinities between respirable air and sulphur, and between vitriolic acid and fixed alkalies, lime, and barytes ; consequently, if sulphur be applied to carbonic acid, united to these alkalies and earths, the affinity between sulphur and respirable air cannot disengage charcoal from the carbonic acid in mild alkalies and absorbent earths. This conclusion would, however, only be just, provided no other agents interfered ; and as we cannot be absolutely certain that they could not, I repeated the above experiments with sulphur instead of phosphorus ; by which I produced a blackish powder, that had the properties of reducing calx of lead, and changing vitriolic salts into hepars : but as it did not burn on red hot iron, and deflagrate with nitre, I cannot pronounce it to be charcoal ; thinking it most prudent to reserve this matter for future examination.

Leicester-Square, September 20, 1792.

P. S. In consequence of some observations published in the *Annales de Chimie*, Juin, 1792, Tome XIII. by Mr. FOURCROY, it is thought proper to add, that although the above paper was not read till May last, it was presented to the Society in March

preceding ; and the experiments were made during the author's autumnal course of lectures in 1791. In these experiments he was assisted by Mr. EGGINTON, of Queen's College, Cambridge, who attended that course. The products were shown, and the experiments mentioned to several Members of the Royal Society the beginning of last winter, particularly to the President, who honoured the author with his attendance during several of the processes.

The substance produced by M. RAYMOND, referred to in the *Annales de Chimie*, is a *humid* combination of phosphorus and lime, and does not decompose cold water ; it is therefore a very different composition from the *phosphur of lime* described in the above paper.